Review of PATH Final Report for Fiscal Year 1998

Saul B. Saila

General Comments-

It is my opinion that the organization, presentation, and explanations of materials contained in this report are substantially better than those provided in previous reports. The editors and contributors are to be commended. I was also impressed by the even handed way that the output from the PATH Scientific Review Panel's weights obtained through the weight of evidence process were treated in this report. The above comments relate only to organization and exploratory material. Comments on specific methodologies are included in the reviews by section which follow.

I was somewhat surprised and even disappointed to recently read an article in Fisheries, Vol. 24 No. 3, March 1999 entitled Fisheries Management - Return to the River: Scientific Issues in the Restoration of Salmonid Fishes in the Columbia, written by a group of authors called the Independent Scientific Group, which includes one member who is also a contributor to the 1998 PATH Final Report. The above report contained no mention of the PATH process nor to the contributions of PATH to Columbia River salmonid restoration. Does this suggest some deficit in communication between the two interested groups?

I also feel somewhat uninformed regarding the ultimate resolution of a response to newspaper stories related to the conclusions of the SRP shortly after the report was presented.

I wonder whether the article in Fisheries Vol. 24 No. 1, 1999 by Soltare et al. entitled Inverse Production Regimes: Alaska and West Coast Pacific Salmon would have influenced the SRP position on climate forcing of some aspects of salmonid production in the Columbia system.

Finally, I still wish to express some personal reservations regarding the Bayesian simulation model and inferences drawn from it. These are related to structural uncertainty introduced by the complexity of the BSM model, which includes the number of parameters and their interactions, and differences in judgments and interpretations by various experts. Added to this, I believe there is considerable additional uncertainty-including the randomness of nature, the accuracy of counts and measurements, systematic counting and measurement error, bias error in making observations, etc. I would like to paraphrase a saying which I seem to recall from the past, namely "We ought to understand simplicity before we can understand complexity." I interpret this as saying a simple and pragmatic model of reality may be most effective. Perhaps the following article, which I found interesting, may assist in providing another perspective. It is:

Schweder, T. 1998. Fisheries or Bayesian methods for integrating diverse statistical information? Fisheries Research 37:61-75.

Reviewer: Saul B. Saila
Title of Paper: PATH Final Report for Fiscal Year 1998
Section 2: Spring/Summer Chinook

Comments:

a) scientific soundness of the methodology

Although I am generally satisfied with the scientific soundness of the methodology used, I have previously commented upon, and still remain somewhat concerned that insufficient attention has been given the treatment of uncertainties associated with model inputs. The Bayesian simulation model also invokes some concerns on my part. These include the fact that the simulation model is very complex in the sense that it involves a large number of parameters. The "bottom line" in this respect is that good performance of a model in the model estimation and calibration phase does not assure correct predictions. I am also concerned about the difficulty of identifying defensible priors. Although the use of so-called non-informative priors does in some sense minimize subjectivity, it does not completely remove it. The problem I perceive is this: if a parameter is non-linearly transformed (as has been done with respect to Ricker spawner-recruit function parameters), then the shape of the prior density is also transformed. For example, if a parameter (p) has a uniform distribution from 0 to 1, then the transformed parameter 0- = - log p has an exponential distribution. The exponential density is not flat any longer and as such, it is not non-informative. This says that the property of being non-informative is not transformation invariant, and thus some element of subjectivity is always present in the prior distribution. How does this affect inferences and projections from the model?

b) general suitability of the data for use in the analysis

The limitations of the data have been generally recognized. However, I have previously expressed concerns about the data used for the stock-recruit relationship, and I continue to be skeptical about the parameters derived from them and their influence on model outputs.

c) validity of inferences and conclusions reached

The validity of the inferences and conclusions reached are dependent on the validity of the model parameters and the model structure(s). Another persistent concern I have is the following question. Is it reasonable to assume that the carrying capacity (productivity) of a given stock and its environment remain constant over extended time periods (decades) as seems to be suggested in the prospective analysis?

d) suggestions for improvements and extensions to the analytical approaches used

My suggestion for improvements and extensions are primarily related to the issues concerning uncertainty in model input parameters and means for propagating that uncertainty in model projections. I believe that alternative methods, such as interval analysis and fuzzy arithmetic, should be considered.

e) opportunities for integration of the different component analyses into an adaptive management approach

I heartily endorse an adaptive experimental approach to management. There are, in my opinion, substantial opportunities for integration of available information and results into such an approach.

f) relative priorities for future work on these analyses

I believe that the assumption that additional sources of mortality act on transported and non-transported fish equally should be tested experimentally, but I don't think that future work on other analyses is justified for this section.

Reviewer: Saul B. Saila
Title of Paper: PATH Final Report for Fiscal Year 1998
Section 3: Fall Chinook

Comments:

a) scientific soundness of the methodology

Since the overall approach is very similar to that applied to spring/summer chinook, the comments made in regard to the scientific soundness of the methodology for spring/summer chinook apply herein.

b) general suitability of the data for use in the analyses

The data for fall chinook are clearly more limited than the data for spring/summer chinook. Its suitability seems to correspond to that for spring/summer fish.

c) validity of inferences and conclusions reached

The preliminary nature of the inferences and conclusions is recognized and specific comments on details are appended to this review.

d) suggestions for improvements and extensions to the analytical approaches used

I believe that a sensitivity analysis of direct turbine mortality is desirable. I also believe that further empirical studies of turbine-related mortality are justified.

e) opportunities for integration of the different component analyses into an adaptive management approach

The opportunities for integration of the different component analyses are limited by the amount of work completed.

f) relative priorities for future work on these analyses

The assumptions for the CRiSP and FLUSH models should be analyzed (and perhaps restructured somewhat) after which a sensitivity analysis should be conducted.

Additional comments related to Section 3. Fall Chinook

1) In Table 3.1.2-9 and Table 3.1.2-10, page 106, I would once again point out that the r2 values for the Ricker spawner-recruit function are less than spectacular. More than one-half of the total values displayed in the two tables are less than 0.50 which indicates that they explain less than 50 percent of the variation in the model. I am concerned about model parameters resulting from this kind of fit.

2) Page 106, first paragraph, last two sentences-They are: "Survival rates were expressed as the natural log of the ratio of observed R/S to the predicted R/S. The natural log of these rates transforms the differences, such that they tend to be normally distributed." Where is the evidence for this? Also, please refer to the comments regarding the scientific soundness of the methodology for spring/summer chinook.

Although data transformation is prescribed to improve additivity, homoscedasticity, and normality, only in some circumstances will it serve these purposes. I believe that without first carefully exploring the data, a transform may hinder more then help subsequent analyses. Based on the mean-variance relationship, if the percentage of error with respect to means can be approximated to some constant, then a logarithmic transformation may seem appropriate.

3) How were the trends in Figure 3.1.2-5 calculated? I calculated the trend for the first panel (namely, the Deschutes River fall chinook) and obtained a negative instead of a positive trend as indicated in the figure. Although the negative trend is not statistically significant, it certainly does not look like the illustration. See my Table 1 which follows on the next page for my results and the data used.

4) Bypass Survival, page 118-Why wasn't the substantial variability quantified?

5) Figure 3.2.1-2-Why should the relationships between the upstream and downstream reaches be so different? The observed survivals seem to suggest a curvilinear relationship for the upstream reach.

6) Discussion, page 123-I think that ignoring uncertainty in the behavioral parameters is unfortunate. Can it be addressed?

7) Fish Travel Time Estimates, page 127-I believe that there may be more effective ways to establish these relationships than the regression technique used. I believe that a neural network approach would be more effective because it can accommodate non-linearity and does not require the strict regression assumptions.

8) Page 135-The assumptions that the number of age 3+ spawners, the proportion of results transported, and total direct in-river and transport mortality are increased without error seems like a lot to me, at least.

9) I do not understand the rationale for assuming e-t and e'-t are independent normal variables (bottom of page 135) and then stating (second paragraph, page 136) "maximizing the probabilities of the residuals e-t and e'-t is equivalent to minimizing unexplained noise, because the probability distributions for e-t and e'-t are normal distributions with a mean of zero." Somehow this reasoning seems circular, or perhaps more explanation is needed for me at least.

10) Page 144-The differences between FLUSH and CRiSP models for migration only should receive careful further consideration, in my opinion.

11) Page 147-The AIC (Akaike Information Criterion) is used exclusively in Table 3.3.2-1. It was my understanding from previous discussions that the BIC was more appropriate for the purposes at hand. Why not use it also?

Reviewer: Saul B. Saila
Title of Paper: PATH Final Report for Fiscal Year 1998
Section 4: Analysis of Effects of Proposed Actions on Snake River Steelhead

Comments:

a) scientific soundness of the methodology

I believe that the methodology is basically sound and seems to be tailored to the limitations of available data. In fact, I believe this analysis seems to provide a rational set of conclusions from the available information

b) general suitability of the data for use in the analysis

It seems evident that the data for this species is more limited than for spring/summer chinook. However, I believe it is appropriate for the types of analyses which were performed and the assumptions implicit in them.

c) validity of inferences and conclusions reached

I consider the inferences and conclusions valid under the assumptions and method employed.

d) suggestions for improvements and extensions to the analytical approaches used

I provide some suggestions for improvements and extensions to the analytical approaches used, and some comments in an appended section of this review.

e) opportunities for integration of the different component analyses into an adaptive management approach

I believe the opportunities for integration of the component analyses into an adaptive management framework are still somewhat limited due to the preliminary nature of this work, and because an adequate review of steelhead life history and management requirements has not yet been accomplished.

f) relative priorities for future work on these analyses

I was particularly impressed by the relevance of three items in the list of future tasks, Section 4.9.2, page 200. These are: 1) Development of passage model inputs, ..., 2) Examine SAR sensitivity analysis..., and 3) Conduct a detailed review of the pros and cons of alternative SAR definitions....

Additional comments related to Section 4. Analysis of Effects of Proposed Actions on Snake River Steelhead

1) Flow versus Juvenile Survival, page 180-It seems to me that the regressions of survival of daily release groups against flow should be examined-perhaps with other regression models or other paradigms. These

responses are used in a prediction sense, and I believe the prediction power of those tested is inadequate.

I also think the relations between SAR for steelhead and water travel time should be reexamined or recast in another framework.

2) Page 190-I have attempted in the case of Table 4.7.1-8 to demonstrate with one simple application of fuzzy arithmetic, the possible utility of this in future PATH-related investigations. This example, illustrated by Table 2 and two accompanying figures, shows that the possibility range of the difference between steelhead and chinook is much wider in the case of the Harman et al. data. It also illustrates what was deduced in the text, namely that there is considerable overlap in the two sets of differences.

3) I also believe a careful comparative analysis of known steelhead life history trends, physiology and breeding biology, and those of chinook should be made. These should then be utilized in further steelhead studies. From a casual examination of Figure 4.4-2, it seems to me the steelhead was in much better shape then the chinook in the past decade or more.

Reviewer: Saul B. Saila
Title of Paper: PATH Final Report for Fiscal Year 1998
Section 5: Sockeye

Comments:

Due to the preliminary nature of available information on sockeye salmon, no effort is made by this reviewer to formally comment on various aspects of the work in progress. It appears obvious, however, even from this preliminary study that the descaling problem is very significant. I also believe that the proposed study of the effectiveness of the captive breedstock is a potentially important area for further work.

Reviewer: Saul B. Saila
Title of Paper: PATH Final Report for Fiscal Year 1998
Section 6: Experimental Management

Comments:

I believe that the description and explanation of the methodology were very effectively presented. I do not consider it necessary or appropriate to follow the review guidelines for this section. There is no question but that the methodology is sound, and the available data and results are suitable for experimental management.

The only suggestion I can make at this point is that the incorporation of the precautionary principle should be explicitly made in the experimental management plan. Although the example provided in the following reference applies to a forest-wetland environment example, it may provide some useful ideas to incorporate into this Experimental Management Section. The reference is:

Rogers, M.F., J.A. Sinden, and T. DeLacy. 1997. The precautionary principle for environmental management: A defensive expenditure application. Journal of Environmental Management 51:343-360.

[Editor's note: This document provided 4/25/99 by Dave Marmorek for posting on the PATH web site.]